Dear Sir Karl,

I have given some thought to the examples ~~given~~ in your paper 'On the Part Played by some Empirical Refutations in the History of Physics'. I am ~~largely~~ in agreement with much of what you say but here are a few comments that occur to me:

Example (9) Perhaps the observational equivalence of the (final) Lorentz and Einstein theories should be stressed — the empirical refutation here is of pre-Lorentzian aether theories. Einstein v. Lorentz seems to me a question of looking at the same observational facts from different points of view rather like Copernicus v Ptolemy. The ad-hoc character of Lorentz's theory has of course been argued for by Zahar (BJPS 24 (1973) 95, 223) and the observational equivalence has been stressed in recent papers by Podlaha and Sjödin. ~~of the~~ ~~Neo-Lorentzians like~~ ~~school~~. ~~For BJPS~~ (see e.g. ~~Podlaha (19~~ Sjödin Nuovo Cimento 51B (1979) 229)

Example (12) Lenard's experiments are ~~certainly~~ referred to in the introduction to Einstein's 1905 photon paper but this was only part of the motivation — the analysis of fluctuation phenomena for radiation as reflected in the ~~Planck~~ Wien limit of Planck's theory seems to have been the decisive point here.

Example (13). Einstein's paper on Brownian motion. only mentions other phenomena in passing – it is often seen I think as a natural development of his ~~theory~~ statistical mechanics papers of 1902 → 1904

Example (18) Anderson's discovery of the positron did not stimulate directly new theoretical advances in quantum mechanics – the theoretical work had already been done by Dirac, Oppenheimer etc. Oppenheimer Phys Rev 35 (1930) 562, 1930 noted ~~that~~ that Dirac's original electron – proton interpretation would lead to a much too rapid annihilation of electrons and protons. – cf. also Weyl's comments on Dirac in his book The Theory of Groups ~~and~~ and Quantum Mechanics, ~~published in 1928~~ (1931), p. 263, English ~~edition~~ translation.

With regard to the Copernican Ptolemy case I agree of course that the motivation of Copernicus was ~~does~~ not ~~so~~ ~~to~~ refutation of Ptolemy the theory observationally. Both Copernicus, Ptolemy can perhaps best be seen as responses to the empirical refutation of the view that the planets should have simple motions as viewed from the earth – ~~from the point of view of heuristics~~ the importance of reformulating a theory in an observationally equivalent way ~~from the~~ is discussed in a paper of mine to be published ~~later this~~ shortly in Studies in

~~2~~, the development of the Feynman diagrams in History and Philosophy of Science, entitled 'Some Philosophical Aspects of Particle Physics'. ~~I~~ For example discuss ~~the case of~~ the so-called second quantization approach to the N-particle Schrödinger equation,*[illegible] and also the rather ~~slightly~~ different case of the development of S-Matrix theory in the 1960's. Here, the arguments were mainly about what ~~sort of explana~~ set of properties of the elementary particles needed to be explained. The S-Matrix theorists like Chew were convinced that mass-values and coupling constants should be predictable; in conventional field theory ~~these~~ ~~some~~ parameters of this kind were put in as a purely contingent feature of the physical universe. So the motivation for S-Matrix theory was not empirical refutation of field theory so much as that field theory was not ~~always~~ devoted to explaining what they, the S-Matrix theorists, thought in principle should be explicable. This sort of argument about what needs explaining, of what constitutes a problem, is crucial also of course in the development of ~~classical~~ Newtonian mechanics in the 17th Century! ~~This~~ Indeed ~~comment was not of course contradict~~ yes in these cases ~~theses that~~ new theories arise in response to problems which are seen as problems in the light of theoretical expectations (over and above the admitted

[illegible]

* Appendix. Remark (d) The motion of the perihelion of Mercury can hardly be said to have been the problem that led to general relativity (~~do you mean Newton rather than E~~ I do not follow the appearance of the word in Newton, 'though this clearly refers Einstein!') The heuristic route to general relativity always was in seeing the identity of gravitational and inertial mass as something that needs explaining

cherry-pickedness of the *empirical* refutation(s)) type of example.

* I enclose a copy of my paper and would be very interested in your comments, — I also my paper on 'Models in Physics' which appears in the current issue of the BJPS.

With best wishes

Yours sincerely

P.S. I have just now received your letter about the two previous papers I sent you. I am sorry to have overlooked your earlier discussion of the distinction between measurement and state preparation — I quite agree you made the point very clearly in Logik der Forschung. I am at present writing a paper entitled 'Causality, Relativity and the EPR Paradox', ~~in~~ I have promised to finish it by 1st November (it is to be read at the Royal Institute of Philosophy Conference at Keele next year.) Then I send you a copy and then we can compare conclusions and try to thrash this whole matter out. I am very appreciative of your interest in my work and look forward to fruitful discussion and collaboration in the future.

I would be happy to look at the paper on Schrödinger's equation. My private telephone number is 01-736-6767 and my home address 34 Gaiger Road London SW6.

PPS. Is there any chance of your coming to the Conference I am helping to organize for the BSPS next week-end. I enclose details of the programme. ~~Sir Brian~~ Pippard's paper on Saturday morning might be of particular interest.